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ETHNOGRAPHIC REALISM AND THE ROLE OF THE ETHNOLOGIST OF RELIGION

To my late teacher Dunja Rihtman-Auguštin

On the basis of his own ethnographic experience in researching a particular religious movement, the author discusses the diverse contemporary concepts of field research and representations of field material. While not disputing the numerous advantages that they have brought to ethnography and its affirmation, their reverse side, possibly limiting to the ethnographer, is pointed out in the text. Going out into the field and entering into the social world of *Others* is stressed as being the fundamental ethnographic activity, which may, but need not, enable the uncovering of unknown processes of social life.

Keywords: ethnographic realism, insider/outsider problem, ethnology of religion

When one permits those whom one studies to define the terms in which they will be understood, suspends one's interest in the temporal and contingent, or fails to distinguish between "truths", "truth-claims", and "regimes of truth," one has ceased to function as historian or scholar. In that moment, a variety of roles are available: some perfectly respectable (amanuensis, collector, friend, and advocate), and some less appealing (cheerleader, voyeur, retailer of import goods). None, however, should be confused with scholarship (Lincoln 1996:227).

The foundation for this paper is the personal, intensive experience of ethnographic research into a particular religious, Roman Catholic movement, initiated in Spain in 1964. The research of the movement, which has some 17 000 communities present today in about 103 countries, was carried out in Croatia between 2000 and 2002, with the purpose of collecting material for my doctoral dissertation. Discussions concerning various contemporary concepts of field research and representations of field material, which I had known only in theory until then, took on a

practical and realistic dimension in the direct field context and showed themselves to be an exceptionally interesting, and even *inescapable*, subject of reflexion. The structure of this article follows my journey – and its analysis – through the diverse paths of contemporary ethnography. In the first part of the article, mainly on the basis of my own experience, I discuss various aspects of today's largely insider mode of ethnographic research, appealing at the same time for the perception of the ethnographic field as the common "creation" of both those being researched and the researcher. In the second chapter, I try to affirm the perception by which the essence of ethnography is going into the "field" and "entering" into the social world of *Others*, thus allowing for the possibility of uncovering unknown processes of social life. Corroborating the stances that the ethnographer is capable of capturing field "reality" and that the purpose of his/her work is not directed exclusively to those researched, together with the application of anthropological principles such as cultural relativism, empiricism and universality, I try to provide arguments for the bolder presence of the ethnologist on the socio-humanistic scholarly scene.

From participant to observer

"Anthropologists don't study villages; they study *in* villages" (Geertz 2001:69). In keeping with the prestige of its author, this quoted thought of Clifford Geertz, first uttered back in 1973, demonstrates extremely well the attitude of the majority of contemporary ethnologists regarding the field and their place within that field. In other words, recent anthropologists are aware that they cannot be completely objective, distanced observers in their research into the "village", but that they are a component part of what they are researching. Although the conception demanding that the researcher enters into the community being researched emerged at the beginning of the 20th century, it attained its full efflorescence only during that century's last two decades, as a fellow traveller of the reflexive turn. This conception of ethnographic work demands almost axiomatically, as "the fieldworker's first commitment" (cf. Emerson 2001b:1), that he/she should "enter" into the researched community and the existing worlds of other people, so as to be able to come to know, personally and closely, their activities. This means that only by submitting him/herself to the social situation that is experienced by the members of the researched group can the ethnographer genuinely penetrate into their mode of response to everything that "life does to them" (Goffman 2001:154). However, there are other reasons for spatial, temporal and emotional bonding with the group being researched that are not without peril. Thus, Maxine Baca Zin (2001) also sees the reasons for the insider approach in avoidance of the lack of trust that human groups show towards strangers, by which the insider-ethnographer can ensure an approach to all events and thus, by cognition of what is really happening, evade the possibility of being deceived by the interlocutor. An indisputable "danger" for the ethnographer "entering" into

the researched community is seen in the development of an emotional relationship and an empathetic identification, which, in a discipline in which the researcher is also a "research instrument" (cf. Cassell 1980:36), can cause considerable methodological problems.

As mentioned above, I first encountered in a more serious form the ethnologist's problem of participation and the setting of its appropriate borders, and its positive and negative sides, in my research into a religious group as the subject of my doctoral dissertation. Since the intention of my work was to uncover the social world of that religious movement and all its ritual activities and notions of the sacred, hitherto unknown to ethnological scholarship, my sole appropriate possibility was field research through participation and observation in accord with the quoted contemporary ethnographic conceptions, that is, the insider approach. It was here that the first obstacles appeared, due to which I have full understanding for those who prefer to deal on a theoretical rather than a practical basis with the insider issue. Namely, all ethnographers are aware that it is not at all easy to enter an already formed group, and even more difficult to become its fully accepted participant. To achieve the entry itself, one needs a host of social skills and "negotiations" with the group to be researched, and these are frequently not at all simple and even unsuccessful, of which I had personal experience on numerous occasions. For example, in this concrete case, my initial idea for the approach to the research was to do comparative research into three diverse religious groups, which are active in contemporary Croatia. Since their more or less official representatives showed no favourable interest, I had to give up the idea. My speedy abandonment of the original intention was also prompted by another limiting factor in the anthropological field – time. Namely, after obtaining my Master's degree in economic anthropology, in accordance with the Croatian Act on Science, I had exactly four years to complete and defend my doctoral dissertation, making it essential that I find a new "subject" as soon as possible. So, similarly to many other anthropologists of religion, I acted opportunely and decided to do research into the religious community that I knew best – as it knew me – and whose full member I had once been.

This fact immediately implied that the field work I would be doing would not be at all classical, that many phases of entry and exit from the field would be blurred, and that my entire field research would be similar in many aspects to an ethnographic experimentation of sorts. All this seemed sufficiently uncertain (or exotic?) and relevant (or modish?) for me to venture into just such research, with the intention from the outset that it not be transformed into auto-ethnography. There are, no doubt, many much better "writers" than I am, and the people in the movement I want to write about, along with the movement itself, are much more interesting than I am, at least to me.

My "entry" into the researched religious community began in 1993, when I accepted the invitation of a female friend from my student days, who was a very committed member at that time. My acceptance of her

invitation was merely a continuance of an interest in religious themes that had developed earlier, ranging from Buddhism through Christianity and Judaism to, for example, fairies and sorceresses. In the beginning, I very rarely attended the religious movement's meetings, one of the main reasons for this being the war in Croatia, in which I actively participated. I was most regular and most engaged in movement during the 1998 to 2002 period, since when I have visited meetings on only a few occasions. Since I started working on my doctoral dissertation at the beginning of 2000, the period in which I conducted classical ethnographic research and wrote field notes, recorded sound and pictorial material, and had informal and targeted conversations, and the like, lasted for approximately two years, from April, 2000 to March 2002. Similarly to other ethnographers, I, too, had to build up my role of researcher in the community through concrete "negotiations" with its members. My task was made easier and, for the same reason, more difficult, by the fact that we knew each other well, since this made it almost impossible for me to be perceived seriously as a researcher. In any case, my participation in the movement proved to be an essential ethnographic activity, since it facilitated both my presence at almost all desired ritual activities, and the fact that I was not rejected as a researcher. Another important factor was the favourable reaction of one of the leading authorities of the researched movement in Croatia, when I introduced my research idea to him.¹

I mentioned above that I had to build up my role of researcher in the community in concrete interaction, in "negotiations" with its members, and those interactions also encompassed constant, interactive redefinition of my field role, from non-participant to active and full participant, from participant observer to observer participant, and from religious searcher to religious *researcher*. Since at that time, due to certain disillusionary events in the community and also to changes in my own views, I was making less effort to reconcile my personal conceptions about the world and life with those that the community offered, interaction in the field often had converse objectives. For example, members of the movement with higher status tried largely to prompt me once again to commit myself more energetically and fully, while I repeatedly rejected such full re-engagement for various reasons. All that notwithstanding, even today when some years have elapsed since I have visited the religious community, I believe that it is most unlikely that the majority of its members will ever perceive me other than as the full member I was at the beginning. This was testified to by a recent encounter I had in the tram with a female member of the

¹ The extent to which my final conception of the research and the dissertation is consistent with my initial ideas presented to the authorities of this movement is another matter, since field information and more recent literature oriented me in a different direction than I had supposed at the beginning. The only explanation I can find lies in the fact that it is almost impossible in a discipline like ethnology to present the planned research precisely and correctly at the outset, for the researcher him/herself cannot be sure of the direction that his research will take him/her.

movement, who introduced me to her young colleague from work, also a member, as a "brother from the community". In any case, interaction in the field and the relations established showed me that it is only through interaction and personal relationships with interlocutors that the ethnographer can "create" the field and "enter" into it. Therefore, I believe that "creating" the field can never be the individual undertaking of the ethnographer alone, since, as in my research, the social group always shows itself to be the "co-creator" that allows the ethnographer entry, sets the borders for the ethnographer, decides on when the research ends, and so on. This is perhaps most readily apparent in the first case – allowing the ethnographer into their own community – since, without that, one *cannot* enter the field, whatever the ethnographer may try to achieve and whatever authority, social power or influence the ethnographer may have.

Participating in the life of the community that I was researching, I took the research position that is supported by the majority of contemporary anthropologists, who are, however, largely in disagreement about what the demanded "being there" means exactly. Some still hold the opinion that one should not be more than a passive observer in the field, while others think quite the opposite, such as, for example, feminist anthropologists, who believe that any field research without full and active participation is morally and cognitively questionable (cf. Emerson 2001d:123). I belong to the group of ethnographers that is not inclined to such a radical division and supports the more fluid approach, which allows the ethnographer to move between the two extremes mentioned – from passive observer to active participant – which I myself practised during the fieldwork in question herein.

I must also point out that, together with Lofland and Lofland (1995:31-41) and Schwartz and Jacobs (1979:57), I share the opinion that the key dimension of the ethnographer's field relations does not in fact lie in the extent of participation, but in the open or enclosed character of the research. As ethnographers, we can participate completely in the life of a human group without fully informing its members about our *real* objectives, while, ethically speaking, the opposite should, of course, be our intention. Our excuses for not doing so can be varied: at the outset, we ourselves do not know exactly what we are researching; it often happens that the members of the group are not at all interested in our objectives, particularly if they are expressed to them in unfamiliar professional terms; this is the only possible way for valuable scholarly research to be done; or, it minimises the reactive effects of the appearance of a researcher in the group, and so on. Regarding my own research, I do not think I sufficiently informed my own interlocutors about its objectives, while the reasons for that are partly present in each of those given above. This particularly relates to the first reason, since I formed the final interpretative model or, more precisely, models, only a year or more after leaving the field. Apart from that, since part of the field material is also autoreflexive, I arrived at some data at junctures when I still was not aware that I would be using

them in my dissertation research and, moreover, had no inkling whatsoever that I would be doing so. However, whether or not the ethnographer fully informs those being researched about what he/she is trying to achieve,² and despite the research position taken, I think it is always important for the field researcher to follow the advice of Bace Zinn (2001:164) on how essential it is that "the informants receive something in return for the information they provided (... because it is) essential to alter the exploitative relationships which research imposes". In my case, reciprocity on this occasion, too, was facilitated by knowing my interlocutors well, many of whom are still pleasant casual acquaintances, so that it was not at all difficult for me to participate with them in various social activities, indeed, one could say quite the opposite.

Thus, the problem that arose in this research – which will take me back to the theme in the preceding paragraph – is of a completely different nature: to what extent is "blending" into the researched community, which includes affective participation and empathetic identification, at all desirable in the field? In other words, whether the ethnographer admits it or not, during each field research project (for example, because of sharing life with the researched community members, entering into their models of meaning and personally experiencing their moral norms) he/she undergoes a resocialisation of sorts, and this, coupled with the attained empathy, can have a considerable influence on the observations and conclusions expressed. The problem is even more complex today because research projects are no longer "at least two-year" undertakings, as in classic ethnography characterised by Malinowski, but have become long-term, almost life-long projects, as in the decades-long research of the anthropologists of religion, Thomas J. Csordas (cf. Csordas 1996) and Jim Birkhead (cf. Birkhead 1999). Because of such prolonged research, it is no wonder that the ethnographer's field role is changeable and constantly moving in the direction in which he/she is an observer on the one hand, and a participant on the other. The traditional analysis of field relations in the notion of roles (Gold 1958, Adler and Adler 1987), the two most frequently applied ones being that of the observer who takes part and the participant who observes, has been largely abandoned today and one speaks of the fluidity of the ethnographer's position in the field, with intervals of change almost from moment to moment.

This case was identical with my research, except that I somewhat more frequently used participation with observation, which Adler and Adler (1987) divide into (a) active participation – in which the researcher participates in the activities of the group and is prepared to accept a certain member role in so doing, but is not prepared to accept all the roles offered nor to devote him/herself permanently and fully to the group; and, (b) full participation – in which the ethnographer does everything as the other

² It seems to me that Duneier (2001) gave a sound example of his good but unsuccessful intentions.

participants do and his/her intention is full and permanent dedication.³ At the time of actually conducting the research, my position, which is perhaps evident from what has been said above, went only as far as the role of an active participant, prepared to accept certain roles, but also to reject others. I rejected those roles particularly that, in my judgement, of course, implied an excessive commitment or too much pressure on my individuality. On a number of occasions, I was concerned that the difference between the community's views and mine would lead to my being banned from the community, which would have meant loss of the "field", but, since my behaviour was tolerated, it was obvious that such a stance was familiar to and accepted by the leaders of the movement.

There were certainly other reasons for my being tolerated and accepted in the community, some of which lay in the personal characteristics that define every ethnographer in the field: ethnicity, gender and age. Since the research was conducted in the capital of the Republic of Croatia and since all my interlocutors were Croatian, that is, of the ethnic group to which I belong, I had no insider problems whatsoever in that regard. My religious past and the varied religious interests I mentioned above were no obstacle, since many other people in the researched community had similar histories as religious searchers. The second characteristic – gender – was perhaps of the greatest advantage during the research. In other words, there were more women than men in the community, so that all male members were treated with more consideration, specially those of about my age, that is, those in their late twenties and early thirties. In respect of age, too, I fitted into the community in which 90% of members are between the ages of 20 and 40.

Scholars from certain disciplines could well ask: by taking part – which has been shown to be the dominant mode of ethnographic research today – did I not call into question the scholarly authenticity of my research? I believe that I did not, and I shall try to explain why. Firstly, the entire research was conducted within the contemporary ethnographic paradigm, and is *legitimate* in that regard. Moreover, there have been several papers on the anthropology of religion written by full participants and all of them have been accepted as being ethnographically relevant. Secondly, I maintained scholarly reflexivity throughout the entire research project with the aid of a classic ethnographic tool: writing field notes, which I usually wrote at day's end and which always brought reflexivity, discernment and a certain shift away from the described events and phenomena. Thirdly, my thesis was written with consideration and application of diverse cultural anthropological literature, for example, about the semiotic theory of religion or ritual studies, by which the possibility of subjective presentation of material and results was lessened. Fourthly, since each

³ Here, too, ethnographers differ, since some become completely "autochthonous" and abandon research, while others, although they are full members, continue their research and plan a return to the academic world.

scholarly work is subjected to the evaluation of the scholarly community, this one, too, was written during contacts with mentors and colleagues, and their criticism, suggestions and attention drawn to shortcomings, so that I also tried in that way to prevent my own "participational subjectivity" and to maintain scholarly authenticity. To summarise the foregoing, as said by Emerson and Pollner (2001), I wish to emphasise that ethnographic participation does not discard restraint and scholarly reflexion, but only delays them until the time of writing the text.

Ethnographic realism and ethnological research of religion

While the discovered "authorship" of ethnography,⁴ together with the "unmasking" of the ethnographer linked to it – is something with which I agree entirely – I am critical (along with Robert M. Emerson and Martyn Hammersley) of the work of some postmodern theorists, who regard the field as the constructed "reality" of the ethnographer, and support the thesis about representation as the core ethnographic activity by which the ethnographer "translates experience into text" (Clifford 1986:115), which he/she presents later to readers. Adherents to the above-mentioned concept have decided to place increasing emphasis on analyses of the presentation technique, and particularly on what is still fundamental to ethnography – its writing. Thus, more attention is being given to observation of the stylistic and rhetoric devices used in the text by the ethnographer, to how he/she assures the reader of the veracity of what has been written, how the title is selected and the text organised, and how metaphors and metonymy are used, and the like.

I regard the establishment of the foregoing facts as important for ethnography and its successful presentation, so that I tried to apply some of them in my thesis. However, I do see a great *danger* in "ethnography" whose fundamental interest would be the reading of texts by colleagues and their textual and representational dissection. I still believe that the essential ethnographic activity is going to the field and "entry" into the social worlds of *Others*, which can, but need not, lead to the uncovering of unknown processes of social life (cf. Emmerson 2001a:IX).

The conception that I defended in my thesis, by which the ethnographer is not the sole creator of "reality", did not postulate representation as the core issue. I was convinced by all the field interactions, and also by the influence of my interlocutors on me personally, that I was not the only "producer" of the field, but that it came about in a joint effort on my part

⁴ When speaking of the "discovery" of the "authorship" of ethnography, I am referring to the "discovery" of the fact that each ethnographic paper is the individual and unique product of the ethnographer, determined by his/her scholarly interests, methodology, personal characteristics but also – *inter alia* – by the relations created in the field. "Field work, to sum up, is both a deeply personal and scholarly project, while a subjective, emotional experience of field work can not only shape the produced interpretation, but also change the very personality of the ethnographer" (cf. Emerson 2001d:131).

and that of my interlocutors. Those are moments in which the researcher feels – as in the case of Dorrine K. Kondo (2001) – that the closing of the gap between him/her and the social world being studied leads to an increase in his/her own inconsistencies, sometimes to such an extent that there is a fragmentation of the researcher's personality and an incoherence of identity. The ethnographer then really becomes the phenomenon being studied, which is indeed the objective of certain individuals. However, the majority of ethnographers in the field wish to maintain a certain distance, and, in such cases, the sole way of preserving personal integrity is to leave the researched community. Something similar happened to me during this field research so that, in a number of situations, for example, when I felt that the pressure on my own individuality, values or fundamental attitudes was too great, I left the field and returned only after personal consolidation.

When presenting the material, I tried as much as possible that the voice of the insider be audible, too, and, since matters of religion are an extremely sensitive theme, I also tried to maintain the anonymity of the group's members, so that when quoting their statements, I mentioned only their gender and approximate age. While ethical deference to the group's members was shown in this way, it was applied to the community as a whole in presenting and interpreting the material, by avoiding any offence or unfavourable influence on the broader social perception of this religious phenomenon. I saw this as my moral obligation towards the researched community and its members.

However, contemporary demands for insider field research, a polyvocal ethnographic product and a relativism of interpretation, apart from many advantages, also produced, at least in my case, a host of limitations and personal dilemmas as regards the presentation of the research results and their interpretation. As I have already stated, insider research does indeed have many advantages for the ethnographer, so many, in fact, that it can be regarded as a necessary condition of research. For example, without the insider approach I could not even have entered the field, while the direct approach to the movement and being part of it, along with personal interaction with its members, made it possible for me to have a taste of its "real" life, to recognise the changes wrought in its members by the movement, and, among other, to perceive the difference between the "imagined" and "concrete" reality. I was unable to write about this latter discrepancy for several reasons. Firstly, because I could have hurt the persons in whom I saw such discrepancies; secondly, because of the anthropological obligation to avoid, at all costs, the possibility of causing harm to the movement and its members; and, thirdly, because my adverse comments could have been understood as being tendentious, as an intentional search for negativity in the phenomenon being researched. In this way, I found myself in an unpleasant situation for an ethnologist. To say nothing about the inconsistencies noticed in individual members of the movement would mean a lessening of the scholarly value of the thesis, and

to speak out about the discrepancies would mean risking unfavourable comments about my own ethics. I decided to apply reduced scholarly criticism, not only, no doubt, to avoid disparaging moral comments but also because of empathy with the persons being studied. Thus, I also found through my own experience one of the shortcomings of the insider approach: becoming close to the community members inhibited me from freely expressing certain criticism, and, probably, because of a certain bias in observation, deterred me from noticing all the flaws. Due to similar objections, the second thing that I had to abandon in my interpretation was a more emphatic and personal perspective of the individual elements and ways in which transformation of the religious movement's members' identities took place, particularly in regard to the role therein of their leaders – the ritual Elders. For these and for other very similar reasons, I must admit that I felt a deep scholarly frustration. Namely, while I was obliged as an ethnologist to respect the voices of the insiders and of interpreters close to them, I did not find such understanding for my relativistic position.

For example, I accepted as credible the insider interpretation that all the members of the movement came to it in answer to a personal call from the Sacred Other, and I accepted the possibility, in keeping with Roman Catholic teaching, that the members of the movement, by actually transforming their identity within it, "grow in faith and realise their real essence". However, no-one shared my semiotic-symbolic view of religion as a transformatory process in which people's notions of the self are reconciled with notions of the Sacred Other so that they, too, become holy. No-one cared about the theory of cultural anthropology, its cultural relativism and the avoidance of any culturocentric statements whatsoever.

The question poses itself to me of what the ethnologist should actually do in interpretation, and how permissible it is for him/her to insist on personal attitudes and those of the discipline. In this concrete case, is the ethnologist at liberty to give an ethnological interpretation of the studied phenomena, particularly if it is *different* from those of the insiders? Taking care not to damage the dignity of the studied individuals and group, I tried to give just such an interpretation, since I did not see the purpose of ethnology producing one more paper that would exclusively promote and represent the voice of the insider. There have been several such papers about the researched group, most of them written with apologetic motivation by the members themselves. Several critical theological articles have also been written about the researched religious phenomenon, some of them even writing about the conceptual mistakes ("the heresies") of this movement. Consequently, what is permissible for part of the Catholic intellectual elite – I am referring to *criticism* – is regarded as dubious when the ethnologist is in question, all because of part of the contemporary ethnographic tenets.

In my opinion, the fact that individual ethnologists, myself included, avoid criticism of the studied group in their works is not merely the

product of their self-perceptions, but rather a postulate of their own discipline. Is it possible for the ethnologist to emphasise his/her own attitudes in the context in which his ability to come to know field reality is brought into question, which would suggest, in fact, that all ethnographic products could be mere fiction? Can the ethnologist emphasise his/her own attitudes in the situation in which an exact view of the role of ethnography and its relation to ethnology or social/cultural anthropology is unclear? I shall first deal with my own view of ethnography and its ethnological role, and later turn to the issue of realistic ethnographical tenets.

The distinct value of ethnographic field work (cf. Emerson 2001a:ix) lies in insistence on the researcher leaving the academic environment, a "resettling" whose purpose is the establishment of direct, close contact with people and the social environments within which they live their lives. "So viewed, the core of ethnography lies in a set of research practices (...) that can generate *discoveries* of new, unappreciated or unacknowledged processes underlying social life" (Emerson 2001a:ix). The idea of insider approach emerged with the intention that the researcher, through personal experience, be enabled the deepest possible understanding of the processes characteristic to the insiders, their local categories and fundamental concepts, and subjectively semantic experiences. However, this approach from within should not be permitted to become ends in themselves, at least not for ethnologists and cultural anthropologists. If they really want to be more than mere ethnographers, their aim should be to try to uncover – while respecting its ethnographic particularities – the relationship between the culture of the social group and the broader social environment and more general theoretical issues. The very specificity of ethnological or cultural anthropological theory, unlike theological or philosophical theory, is the fact of their being founded on experience (field work), so that it is possible that the results of certain field research could bring into question ethnological theory. This is not meant to infer that ethnographies themselves are free of theory. As Emerson states (2001c:28), with which I agree: "any and all description is inevitably *partial and selective*" and "theory is always 'inherent in ethnography'". It would seem appropriate here to point out that this is also the case when ethnography is produced by members of the group writing about themselves, since the diversity of their personalities also leads to diversity in their representations. I noted during my research, to mention just one example, that there are great differences in the description of field occurrences and their representation between the religious movement's members who are higher or lower in its hierarchy. I hope that fact clarifies why I now think that one can say that "multiple descriptions of the same scene, activity or culture are both likely and legitimate" (Emerson 2001c:30), and that "the ethnographer's accounts are in no fundamental way different from those members provide. Both reflect describer's purposes at hand; that the ethnographer's purposes are perhaps more 'theoretical'

does not make her descriptions any less partial, selective, or perspective than member descriptions – only different" (Emerson 2001c:38-9).

If the purpose of ethnology really does lie in the discovery of "new, unappreciated or unacknowledged processes underlying social life" (cf. Emerson 2001a:ix), then from the ethnological position, unlike that, for example, of literary criticism, I believe that it could be said that the purpose of ethnography lies in providing the material necessary for the authentication and formation of general ethnological and anthropological theoretical concepts. Thus, the ethnologist will not limit his/her activity to attempts at representing social reality, with the aid of the internal knowledge gained, but will also try to give his/her *interpretation*, both from the aspect of an insider and from his/her own discipline. On the example of interpreting the emergence of the religious movement, the ethnological interpretation in my case showed itself to be different from that of the insiders, presented by the founder of the movement. Therefore, I presented both interpretations to the reader, so that he/she, as the postmodern final creator of the meaning of the text, could encounter them on an equal basis. In any case, I believe that interpretation is where ethnology starts to differ from ethnography, and it seems to me that Geertz – speaking of how the purely emic description, even if it were possible, is uninteresting and useless to science – is of the same opinion. He says that our task is to "grasp concepts which, for another people, are experience-near, and to do so well enough to place them in illuminating connection with experience-distant concepts theorists have fashioned to capture the general features of social life" (Geertz 1976:224).

However, for the ethnographic well-foundedness of anthropological theory – which, unlike ethnographers, anthropologists and ethnologists aspire to – to have any scholarly justification, it would be necessary to give a reasoned response to postmodernistic criticism of the realistic tenets of ethnography. Although it is, no doubt, already obvious, it should be mentioned that my position in this discourse is far from neutral. I wrote my entire doctoral dissertation with the intention of supporting the conception in which the essence of ethnography lies in going to the field and uncovering new social worlds in our midst – those that disclose the unknown in what is close and what is close in the unknown. In ethnographic divisions, I shall certainly be characterised as a neo-realistic ethnographer, that is, as the ethnographer who assumes the existence of at least some realistic premises in ethnographic research. However, I believe that the arguments for taking such an ethnographic position are convincing and I shall try to present them, commenting firstly, in part, on the specificity of my own research.

It was done in the classic ethnographic field in which the researcher goes into a natural social environment and the authentic context in which people live, while the difference – nothing new to a Croatian ethnologist – was in relation to the research venue, no longer a distant Croatian village but an urban Zagreb community. As far as field methods were concerned,

I largely used participation with observation, since it made it possible for me to obtain the most reliable data. Namely, with the involvement of such an intimate theme as piety, I realised during my research that I could find out more by observing and listening to people in their spontaneous reactions or during ritual events, than in classical interview situations. So I experienced something similar to what Jasna Čapo Žmegač encountered during her research into the resettlement of people from Srijem, of which she wrote: "My partially insider position along with frequent stays in the field made it possible for me to meet the inhabitants in informal, everyday situations, in which people, at least for a moment, forgot the reason for my presence. These situations proved to be key in certain parts of interpretation, since they revealed the *one-sidedness* (emphasis G.P.Š) of narrative sources in studying processes of identification" (Čapo Žmegač 2001:46).

However, to justify the fact that, among others, Jasna Čapo Žmegač and I were able to come to such conclusions on field research, it is necessary to make the case for the ethnographic capability of grasping field reality. The first defence of ethnographic realism can derive from what I consider to be the incontestable fact that, viewed epistemologically, ethnography is an inductive science, that is, one of those fields of scholarship that arrives at its concepts and theories by generalising individual, empirical data. The extent to which ethnography is, indeed, an inductive science can be seen in that many ethnographers go into the field without any hypotheses whatsoever – ethnomethodologists, for example – expecting the information itself to lead to theory (for example, supporters of the *grounded theory* approach). On the other hand, the trend today (Emerson 2001e, Duneier 2001, Katz 2001) is for the ethnographic method no longer to be regarded only as induction, but rather as *retroduction*, a combination of sorts of induction and deduction. The supporters of this conception say that the ethnographer in the field constantly moves from observation to analysis and back again, since the uncovered data form the theory which he/she applies, and the theory, on its part, orients the collection of data. So this is a case of a two-way complementary process, which is neither induction nor deduction – but, instead, *retroduction*.⁵

Still, all the foregoing assumes the existence of minimal realistic pre-conditions for research, and the arguments put forward by Martyn Hammersley, and Jaber F. Gubrium and James A. Holstein, seem the most effective in their defence. I shall start from the stance that enabled the blooming of relativism, that no realistic cognition whatsoever is possible, due to the cultural and social determinants of all our comprehensions and perceptions (cf. Hammersley 2001). This stance is indeed incontestable,

⁵ As far as I know, the first indications of such a mode of work in Croatian ethnology were seen in the the papers of Olga Supek: "Od teorije do prakse i nazad", *Narodna umjetnost* 13, Zagreb, 1976 and "Nacrt istraživanja jednog prigorskog sela", *Narodna umjetnost* 16, Zagreb, 1979.

but one must ask where we are led by the idea that ethnography is only reality created by rhetorical means, and that, because of relativism, it is only one of many possible realities. The answer to this is inherent in the philosophical question about how much value should be given to the claim that all cognition is relative, since, if we apply it to the claim itself, we find ourselves in an enclosed circle with a loss of all meaning. Therefore, according to Hammersley (2001), the essential position is the "middle way", located between radical realism and radical relativism, which will enable us multiple representation of realities, while each one will be backed up by attitudes that will make certain representations relevant, and some others not. Consequently, in keeping with the conceptions of ethnography, not all representations will be ethnographic; some will perhaps be sociological or archaeological, while some will not even be at all scholarly. A question for some critics of the ethnographer's reality, as already mentioned, could also be: if various perceptions are permissible, while shouldn't the ethnographer's be acceptable as relevant?

Martyn Hammersley summarises his argumentation of realism in three points: the first states that we must not approach knowledge absolutely as "beliefs whose validity is known with certainty" (Hammersley 2001:108). Since nothing can be known with complete certainty, we need to comprehend knowledge as "beliefs about whose validity we are reasonably confident" (ibid.), while we will be reasonably convinced by comparing various possibilities, in which process some of them will prove more acceptable.

Secondly, realism must reject the claim that phenomena exist independently of our statements about them, which we can, therefore, present with accuracy. Since we ourselves are a part of reality, it is impossible for us to see phenomena completely independently. The claim that we can become cognisant of completely independent phenomena should therefore be replaced with the statement "that our making of a claim does not itself change relevant aspects of reality in such a way as to make the claim true (or false)" (Hammersley 2001:108). In this way, the claim that it is possible to become cognisant of phenomena in complete independence does not hold, but, in the same way, nor does the one that cognisable phenomena are completely formed by us.

Thirdly, ethnographers aim at a *representation* of reality, not its mere reproduction. As has been mentioned, representations can differ, the insider representation has been written from the insider point of view, the psychological from the psychological, and the ethnological from the ethnological, and each viewpoint is relevant in its own category and according to its own criteria, this making possible diverse descriptions of one and the same phenomenon.

Jaber F. Gubrium and James A. Holstein (1998) suggest on their part that we can defend realism with the reflexivity of our own research and/or by limitation of reality with the aid of monitoring the ways in which

it is constructed during research. By this method, we obtain qualitative research with elements of both realism and relativism, in which interpretation and its conditions mutually shape each other.

Summing up the few foregoing paragraphs, one could say that representation of reality "as it is" is simply not possible. Insiders are the ones who directly experience the reality that ethnologists want to represent, but what they offer in verbal form to ethnographers in the field are only interpretations of reality. On their part, the ethnologists create interpretations of these interpretations, and, in order that they be as relevant as possible, they themselves become insiders in various ways. The existence of diverse interpretations of reality is not regarded as something that threatens reality, but is rather seen as the consequence of differing observations of that reality. For example, I did not receive any criticism at all of the ethnographic description of the researched religious phenomenon – "insight into reality" – which is not surprising, because I relied largely on the descriptions of the participants themselves; all the objections referred to interpretation – "interpretation of reality". I did not regard the latter as problematic since varying views and interpretations are very acceptable in contemporary scholarship. For the same reason, I see any claim that there is only one – usually one's own – correct interpretation of a particular phenomenon as being a very ethnocentric and, thus, unacceptable attitude. I am particularly averse to monopolisation of interpretations in the field of religion, even if expressed by participants, because I believe that it undermines the very foundations of my discipline of ethnology or social/cultural anthropology, and of scholarship as a whole. In other words, to support the attitude that only members of a particular religious group – usually its intellectual elite – can correctly interpret all the manifestations (including the social and cultural) of a phenomenon of which they are a part means, to an extent, to promote a return to the pre-modern status of scholarship, in which one's own religious truths were the basis of all forms of "scholarly" interpretation.

Because of their powerful desire for social influence, religious organisations, regardless of denomination, always try to achieve "epistemological or cultural authority", that is, "permission to define reality for others" (cf. Hufford 1999:298), while our role as anthropologists or ethnologists is definitely not to oblige them in this intention. Namely, the emergence of anthropology as an academic discipline at Oxford University in 1884 and its beginnings (with the work of Edward Tylor, James Frazer, Emile Durkheim, William Robertson-Smith...) were strongly linked to the search for empirical, comparative, rational and *non-religious* answers to the question of Humankind, its social organisation and cultural phenomena. If contemporary anthropologists were to be reduced to mere transmitters of participant concepts they would lose the specificity of their own discipline's approaches. Furthermore, I am convinced that the social anthropologist is not called upon to be either an "advocate" or a "cheerleader" (cf. the quotation at the beginning of this article) of the researched

(religious) group, but rather a scholar interested in the cultural or social manifestations of a (religious) phenomenon, constant in his/her ethical consideration towards those being studied and uninterested in (meta-physical) questions on the veracity or otherwise of their (religious) conceptions. All this is what I, too, aimed at in my own research, believing that the finest ethnographer is the one who manages to identify with the members of the researched group, while the finest ethnologist or cultural anthropologist is the one who can rise above the subjectivity characteristic of group members and observe the phenomenon without bias in its dimension that is universal to all of Humankind.

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ETNOGRAFSKI REALIZAM I ULOGA ETNOLOGA RELIGIJE

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SAŽETAK

Polazeći od vlastita etnografskoga iskustva istraživanja religijskoga pokreta autor u tekstu raspravlja o različitim suvremenim koncepcijama terenskoga istraživanja i predstavljanja terenske građe. Ne osporavajući brojne prednosti koje su ove, uglavnom postmodernom i literarnom kritikom inspirirane koncepcije donijele etnografiji i njezinoj afirmaciji, autor upućuje i na njihovu drugu, za etnografa otežavajuću stranu. Sredstvo su mu za to, između ostaloga, rasprave o pozitivnim i negativnim stranama *insiderskoga* načina istraživanja, etnografova vlastita tekstualna razotkrivanja i relativiziranja stvarnosti i terenske i one finalnoga etnografova proizvoda. Kao jedan od mogućih putova izlaska suvremene etnografije iz svojevrzne krize identiteta autor sugerira barem djelomično vraćanje postavkama realizma i etnografiji kojoj je bit nastajanje na terenu, smatrajući da to u konačnici može uroditi snažnijom prisutnošću etnologa na društvenohumanističkoj sceni i njegovim argumentiranim raspravama, primjerice o religiji, s drugim akterima.

Ključne riječi: etnografski realizam, problem *insajderstva/autsajderstva*, etnologija religije